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Vol. XII
No. 5

PSYCHOLOGICAL REVIEW PUBLICATIONS

November, 1910
Whole No. 52

THE

Psychological Monographs

Some Mental Processes of the Rhesus Monkey

BY

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THE REVIEW PUBLISHING COMPANY
41 NORTH QUEEN ST., LANCASTER, PA.
AND BALTIMORE, MD.

PRESS OF
WILLIAMS & WILKINS COMPANY
BALTIMORE

CONTENTS.

Introduction[1]

Though one of the newest of the sciences, comparative psychology, or, speaking more strictly, animal psychology, may properly claim as its father that acute observer and many-sided Greek scientist and philosopher, Aristotle. The works of this pioneer in science show him to have been an interested and critical student of the mental, as well as the more strictly zoölogical (morphological and physiological) side of animal life, of which latter sciences (morphology and physiology) he is the acknowledged father. His observations on the comparative mental and moral traits of man and the lower animals, so striking when we consider the data he had at hand, may well entitle him to the credit of being the founder of comparative psychology.

Within the past century, the evolution and anecdote schools, represented preëminently by Darwin, Huxley, Romanes, Lubbock and their co-laborers, have contributed largely to this work and their observations and critical considerations have thrown much light on the mental capabilities of animals. These men, however, depended too much on a method we now believe to be of little value in comparative psychology, viz., observation uncontrolled by experiment, and their critical considerations have been colored because they were interested chiefly in the theoretical, evolutionary interpretation of their observations. This work, however, has been invaluable in that the broader questions have been set, and especially in that it has given great stimulus to the study of the animal mind.

It has remained for the new, the conservative, experimental, school of comparative psychology, to define more

[1] The animals used in this research were supplied by Prof. Shepherd Ivory Franz, of the George Washington University, through a grant to him from the Carnegie Institution of Washington. Acknowledgment is, therefore, made to the Carnegie Institution, without whose aid the work would not have been possible.

accurately the problems of the science, and by more accurate observations and by carefully controlled experiments to solve those problems. To this more scientific and growing body of workers, to Morgan and Hobhouse in England, to Forel, Bethe and Hatchet-Suplet on the Continent, and to Mills, Thorndike, Yerkes and Watson in America, must be paid the highest tribute.

As results of the first part of the work of this newer school distinct problems have been more precisely formulated by the investigators. These problems may be divided into two general classes, each of which may be resolved into a number of concrete special problems. The first general problem is: Do animals possess the lower mental powers that man possesses? Do they have the same fundamental psychic states that man has; and, if so, how do these states differ qualitatively and quantitatively from those of man? To be more concrete, we may ask: Do the lower animals discriminate sensory qualities, do they discriminate brightness of lights, do they discriminate colors or hues, noises, smells, and tactile stimuli? Do they form and inhibit habits, do they retain impressions and have the elements of memory? In what way, if at all, do these differ from similar mental powers of man? The second general problem is: Do the lower animals have the so-called higher powers? Have they ideas, have they the ability to learn by imitation, have they general notions and reason? From any psychological consideration we may omit the question of the presence of moral judgments, a subject which is chiefly of ethical and religious interest.

Despite both the newness and the difficulties of the problems and the imperfections of many of the present experimental methods of study in this field, much work has been accomplished and many valuable results have been obtained. The results are, however, yet too few, and some of those of more general interest have led to unsettled controversies. Some of the results which at present may be considered established are as follows: Some animals, the higher forms at least, discriminate brightness values. Some classes of animals discriminate pitch. All vertebrates and some invertebrates show an ability to form habits of reaction to stimuli, and they form at least simple associations. Animals retain impressions and have a memory of some sort. Even the most conservative and hypercritical have found this to be the only satisfactory interpretation of the results of experiments, and it is almost needless to say that the adherents of the so-called anecdote school admit a high degree of memory in all mammalia.

The question on which comparative psychologists are divided are as numerous as may be expected in a growing science. Those of most interest in connection with the present study may be briefly mentioned. Have animals the same sensory equipment as man? Have animals color vision? Or, do they merely appear to discriminate colors because of differences in the brightness values of the stimuli that are used? Do animals learn by inferential imitation? Have the lower animals ideas or mental images?

In answer to these questions animal psychologists have arrived at directly opposite conclusions, although all the recent investigators believe they follow the law of parsimony enunciated by Lloyd Morgan. In regard to the so-called higher mental powers, recent experimenters have taken a less decided stand than formerly. They cease to deny to animals reason, imitation and other similarly complex processes, but they say the case is not proven and demand additional experiments and observations.

Among experimenters on animals Hobhouse is almost alone in claiming that animals have true general notions. It is true he does this after having made rather important qualifications of the term 'idea.' The latter topic has, however, not been the subject of many experiments or, rather, it has not been the subject of many reported experiments and observations other than a few illy-controlled ones. The question of the ability of animals to reason has been the subject of numerous tests; but the casual observations of the anecdote psychologists are the mainstay of those who support the view that animals show ability to reason. Hobhouse has made some experiments which he believes indicate some power or inference in monkeys and apes, but the observations that have been made by others of the group of experimental animal psychologists are in the main of a decidedly negative character. With the exception of Hobhouse, it may be said that the consensus of opinion is that the presence in animals of this so-called higher faculty is not proven.

These points we may summarize by saying that at present comparative psychologists admit the possession by animals of most of the lower powers of intelligence that man possésses. Such powers of animals may, however, differ from those of man quantitatively and in a qualitative manner. Possession by animals of the so-called higher powers is an open question, or set of questions, which have not been so widely studied, nor so clearly determined; and, respecting which the available evidence points toward a negative answer in the form of a verdict of 'Not proven.'

On account of the ease of obtaining the domesticated animals a large amount of the experimental work on animal behavior has been performed upon cats, dogs and chicks. On the other hand, the lower forms (invertebrates and non-mammalian vertebrates) have been extensively studied in respect to the simplest powers, such as tropisms. On account of expense, difficulties of care and the apparent complexity and variety of behavior, the higher mammalian types, especially anthropoids, have not been studied so consistently.

However, for studies in comparative psychology the value of using primates instead of the lower vertebrates and invertebrates is almost self-evident. On the one hand, the anatomical similarities between man and the monkey are apparent. There is the well-known similarity between both their peripheral and central nervous systems; and the similarity of arrangement of muscles and bones, especially of the extremities, must not be forgotten. Observations have made it evident that there are also similarities in a physiological way. Movements and reactions of an apparently human type are known to be present in these higher forms, which are difficult if not impossible to observe in the lower forms. We may therefore expect from the careful studies of the mental states of those animals, admitted to be immediately inferior to that of man, more light on certain problems in the psychology of human consciousness than from similar studies of others. In making such a comparison of values it is not intended in any way to detract from or to belittle the studies on the lower forms. The value of experiments with the lower orders is everywhere admitted. But at present it must also be admitted that the latter have received the attention of many more writers, and their reactions have been subjected to a much finer analysis.

Experimental

The brief historical summary of comparative psychology which we have given will, perhaps, appear as a sufficient justification for undertaking further study in the field; both in regard to the lower and to the higher faculties in the animal mind. This study of the psychology of monkeys was undertaken, therefore, with a view to add to the knowledge we now possess of the lower faculties in the animal mind, and if possible, to throw some light upon the question of whether or not monkeys possess some of the higher mental powers, and also to limit or characterize any such higher powers, if they should be found. The experiments have been much too incomplete to be entirely satisfactory to me, and, in many cases, the results are naturally lacking in definite conclusions, though they yielded, to me at least, some valuable indications in the field. It must be said, however, that the unsatisfactoriness and the indefiniteness are not peculiar to this particular study, but that they apply to any work that is performed in such a complex science. In certain of the later experiments, it will be noted, definite conclusions have been arrived at, and, had time permitted the carrying out of further experiments, it seems probable that other tests might have been concluded in as definite a way.

As tests of some of the so-called lower faculties of the monkey's intelligence, experiments were made on brightness discrimination, color discrimination, auditory discrimination, the formation and inhibition of habits, and retentive power (memory). In regard to the higher powers, observations and experiments were made on learning by imitation, on ideation, on reasoning, on adaptive intelligence and on general notions. In brief, the chief aim of the study has been, while profiting by the work of preceding investigators, and with a steady adherence to the law of parsimony, to study some of the lower powers of the animal mind, but especially by a modest study of the higher powers to seek for some light, however little, on the subject of mental evolution.

Eleven monkeys were used in this work. All were Rhesus (Macacus) obtained soon after their importation from India. So far as we could ascertain they were eight to nine months old when received and, with the possible exception of monkey 2, they appeared to be without training of any kind. Monkey 1 was a rather large, spare male, moderately active and bright looking; in his work he showed only medium capacity; when he could not perform an act and thus get food he showed signs of anger, by jumping about and by shaking the wire sides of the cage; he would sometimes jump at the experimenter and visitors who happened to be in the room, but it must be remembered he was in a cage and could not get at anyone; the movements he made were those of intimidation, so often noticed in the Rhesus and other Macacque monkeys; towards the end of the series of experiments he became cross. Monkey 2 was a medium-sized male; moderately active and of only ordinarily intelligent appearance; he showed a mediocre capacity in work; not friendly; he wore a collar about his neck when received and this would indicate that he may have been accustomed to handling by sailors or by others who previously owned him. Monkey 3 was a small female, apparently the youngest of the eleven, very active, alert; good-natured and friendly for a Rhesus; formed associations the quickest of all the animals. Monkey 4 was a medium-sized male, not very active, but bright and cunning looking; good-natured; jumped at experimenter half playfully; of medium capability in his work. Monkey 5 was a large male, dull looking, not active; slow in learning as compared with all the others; he had a way of whining when left alone; was rather ill-natured; would get angry when he failed to do a required act, and thus fail to get food. Monkey 6 was a small female; bright and active; good-natured; very cautious in all her work and actions. Monkey 7 was a large female; inactive, not very bright looking, but the most friendly of the eleven animals; proved to be quick in forming habits; very quiet. Monkey 8 was a large male; he was the master of the band; of gruff appearance, domineering, active; of not especially intelligent appearance; but was extremely cautious, appeared to like to go through the tests. Monkey 9 was a medium large female; rather friendly; quick to form habits. Monkey 10 was a large female; not active; quite friendly; fairly capable as indicated in the tests. Monkey 11 was a large male; wise looking; quiet and friendly; somewhat timid; of medium capability.

Formation of habits.

Releasing fastenings: Since the appearance of Thorndike's *Animal Intelligence* in 1898 the puzzle box method has been a favorite one for comparative psychologists in studying the formation of associations[1] by the mammalia. Partly as preliminary to succeeding work on visual discrimination, an experiment similar to those of Thorndike was made with monkeys 1, 2 and 3.

A box 2 feet long, 7 inches wide and 8 inches deep was made with wooden slats and attached to one end of the cage in which the three animals were kept. A solid wooden door, 6 by 4 inches, was arranged on each side of the inner or partition part of this box nearest to the cage. These doors had vertical hinges and were kept shut by a turn button on the side away from the animals. The hinged portions were 16 inches, the opening or button portions 8 inches apart. A space was left on the sides of the doors next to the button sufficiently wide to enable a monkey to put his hand through. The doors opened inward into the feed box, *i.e.*, outward from the monkey cage. The food was usually so placed on the floor of the food box that the animal could not see it, except by going to the extreme right or left, depending upon

[1] The term 'association' as applied to the intelligence of animals has been loosely used. Two principal usages are: (*a*) the animal's simple cerebral (or mental) processes (merely anatomical, or it may be, physiological) such as is shown in a simple adjustment to a stimulus, *e.g.*, in its learning to turn a button to open a door; (*b*) as explaining what are thought by some psychologists to be higher mental processes, *e.g.*, ideation or reasoning. When used in this paper the word is to be understood to have a connotation similar to the first meaning given above.

the box into which the food was placed. In all experiments, even in those in which no time is recorded in the paper, the times for the performance of acts were taken by a stop watch and recorded.

By reaching through the space or crack at the side of the door and turning the button, the monkey could open the door and secure the food which was placed in the food box behind one of the doors. In the beginning of the work only one of the doors was used; when the association of turning the button, *i.e.*, of opening one door, was formed, both doors were used. In the latter (two-door) preliminary experiments the food was placed in a chance order behind one of the doors, the animal not knowing which door that might be.

The records of the three animals used in this first experiment are as follows: On the fourth day, after 32 trials, monkey 1 gave evidence of the formation of the habit of opening the door. Previous to this trial he had shown many random movements, going from one part of the cage to another, getting upon a bench in the cage, stopping all work for a time, etc. On the fourth day, however, the habit of going to the door directly was apparently formed, and the method of opening was that of vigorously shaking the door so that the button was gradually moved to permit the opening of the door. On the fifth day, after 53 trials in all, he began to fumble with the button in addition to shaking the door. He continued to use both of these means with an increasing tendency to rely on the button alone. On the sixth day, after 95 trials in all, he had formed the association of opening the doors by turning the buttons alone. His average time for 10 succeeding trials on this day was 3 seconds. Monkey 2 formed the association of opening the door in much the same manner as Monkey 1. He pulled and shook it, and on the third day, after 21 trials in all, managed to get the trick of always opening it in this manner. On the fourth day, after 51 trials in all, he began to turn the button in addition to pulling and shaking the door. Also similarly to the actions of Number 1, he continued to use one or both of the means, with an increasing tendency to turn the button alone. On the sixth day, after 102 trials, he always used the button alone as the means of opening the door. His average time for ten successive trials was 1.5 seconds. Number 3 on the second day, and after 11 trials in all, formed the association of opening the door by pulling and pressing it. On the third day, after 34 trials in all, she began to turn the button. On the fourth day, after 64 trials in all, she appeared to have perfected the latter association and had inhibited all tendency to use other means (*e.g.*, shaking) for opening the door. Her average time in the last 10 successive trials on this day was approximtaely 1.5 seconds.

The method of learning in this experiment appeared to be of the 'trial and error' type. As has been mentioned, the animals scrambled about in a general way at first; they pulled and bit at the door and the adjacent parts of the partition; shook the door violently; but, *it is to be noted that after the first few experiments the attention of the animal was always directed to the door.* Even after an accidental success in turning the button, the association was not at once set, but only after a number of trials, with accidental successes many times repeated, alternating, or rather interspersed, with many errors. All the animals used in this work appeared, however, to recognize a chance success and profited by it more quickly than did raccoons under similar conditions. Another thing of particular notice in their work, in which respect also they differed from the raccoons, was their tendency to show signs of anger when they could not open the door and get the desired food. This was especially noticeable with Monkey 1.

It is also of interest to note that when, preparatory to another trial, I attempted to close the door after an animal had opened the door and obtained food, the animal would repeatedly attempt to hold the door open, as if the idea was present that 'door-being-open' meant food. This reaction is somewhat similar, I judge, to one observed by Thorndike in his experiments with cats. It will be remembered this author found a tendency on the part of the animals to walk into the opened cage from which they had just previously escaped to obtain food. The action, on the other hand, may be merely of the nature of a *reflex*.

Visual discrimination. It was formerly supposed, or rather taken for granted, that animals possess the power of discrimination of visual qualities, hues, and saturations. This assumption was based upon observations of general behavior, of reactions to objects of different color, etc., but in the past few years, however, comparative psychologists have concluded that such power of discrimination could not be assumed and they have sought by carefully controlled experiments to test the truth of the matter. While the experiments already made have yielded many valuable and apparently positive results, several investigators, among whom may be mentioned Yerkes (4) and Watson (2), await more proof of such discrimination ability. The problem has been resolved into two questions. Is there a discrimination by animals of brightnesses or intensities? Do animals discriminate objects by their color or hue qualities? In the work on visual discrimination to be recorded in this paper nine monkeys were tested for color discrimination and six for brightness (or intensity) discrimination.

Brightness Discrimination—Simultaneous Exposures: Some experiments on monkeys 1, 2, and 3 were made with the same apparatus that was employed in the preliminary experiments on the releasing of fastenings, with the addition of two cards, respectively black and white, which were placed above the doors. The cards were 5 inches square, and were placed above the doors. They were, therefore, 8 inches apart. A piece of food, usually a half or a whole peanut was dropped in the food box; the black card was placed above the door behind which the food had been placed, and the white card above the other door. The monkey was to open the door under the black card and obtain the food. The food was placed in the compartments in an irregular order, and the cards were correspondingly placed. At times the food was not placed in the box until the door had been opened by the animal, but often it was dropped in the box immediately before or after the buttons were adjusted. In this way it was possible to prevent the animal knowing which door to open from the sight of food, and since raw peanuts were used, the smell component, judging at least from human ability to smell such food was at a minimum.

These experiments were begun December 4th and continued for eleven weeks. Each animal was given from 10 to 25 trials every second or third day. In some cases there were longer intervals between the work periods. Each animal was given a total of 1000 trials. At the end of the work all were substantially perfect in opening the door under the black card *first.* No animal was ever able to inhibit altogether the tendency to open the door under the white card after the door under the black card had been opened and the food had been secured. The average time for opening the door under the black card in the last 25 trials for these three monkeys was approximately one second. The curves of learning, *i.e.*, the time-experiment curves, correspond to those found by almost all experimenters, but on account of mistakes in opening the second door they cannot have much value in this connection.

The fact that the animals formed the habit of opening the door under black first would indicate that they discriminated the cards. The quickness with which they finally performed the act would also confirm this conclusion. Moreover their looks and actions at the time appeared to indicate a knowledge that the opened door meant food. When, for example, food had been not placed in the box, after opening the door they would look at me as if they expected their reward. The closeness of the door buttons, and the great activity and ready use of the hands, explain, I believe, the opening of the second door so often. Furthermore, it was noted that while after they had opened the door under the black card, they appeared to expect food, when the door under white was opened, their actions did not indicate that they expected to be fed.

Brightness—Successive Exposures. For the tests with monkeys 4, 5, and 6, I used a card displayer similar to that used by Professor Cole and myself in brightness and color tests made on raccoons. This was placed outside and about 6 inches from the animal cage. The accompanying figure is a diagram of the experimenter's view of this piece of apparatus. (See Figure 1.)

The front of the displayer was formed by a board 12 inches high. A pin, 'P', on which two levers could be turned, was inserted in a hole at the back and near the lower edge. On this pin two displayers, 'W' and 'B', were arranged so as to be freely movable in the plane of the board. The two cards, black and white, were placed at the upper end of the displayers so that the raising of the displayers showed the black and white cards respectively. The card displayers were 1-inch thick so that there was a difference of 1 inch in the distances of the cards from the animal box. On alternate

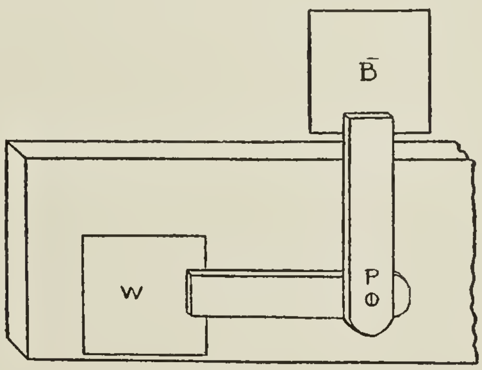

FIG. 1

days, however, the cards were changed from the front to the back lever, so the animals should not react merely to the distance position of a card. When the black was exhibited the animal was to go upon a platform arranged inside the cage and was fed, while he was not to go up at white and was not fed. Usually and except at the stimulus, the animal sat on a bench 12 inches from the floor and inside the cage. He was, therefore, in a position to look down at the card apparatus, for the upper portion of the card was about 5 inches below the horizontal level of his eyes. When the black card was displayed and the monkey had climbed to the food platform the experimenter rose from his seat and presented a piece of food to the animal. It is of interest to note, therefore, that there was no possibility of the formation of an association between the smell of food and food, rather than between the appearance of the card and food. Since the food was not placed near the cage or near the food platform until the appropriate response was obtained the animal had no smell stimulus for its guidance to an appropriate reaction.

These experiments were begun February 5th and continued for a period of seven weeks. Each animal was given from 10 to 25 trials daily, with intervals of two days, in some cases of three or four days, during which intervals no work was done with them on brightness. In all, each was given 700 trials. At the close of this work monkeys 4 and 6 appeared to have the association perfected. The average times for responding by going upon the platform when black was displayed in the last 25 trials were 1.5 seconds for monkey 4 and 1.25 seconds for monkey 6. The tendency to climb to the food platform when white was displayed appeared to be entirely inhibited. Monkey 4 was, in this regard, perfect in the last 25 trials, and monkey 6 responded to white only once in the same number of trials. Moreover, at the time of this mistake it was noted that in the trial (the 7th) in which she responded to white, monkey 6 immediately came down from the food platform with all the appearance of having knowledge that she had made a mistake; she did not appear to expect food and did not remain upon the platform for a sufficient time to have food presented to her.

Monkey 5, at the end of the 700 tests, appeared to be almost perfect. In responding to black he made only one error in the last 25 trials. His inhibition of the tendency to respond to white was not quite perfect, for during the same series he responded to the white card three times. His errors in this latter regard, however, were rather of the type shown by monkey 6. He showed by his actions that he knew he had made a mistake, for without waiting to be fed he immediately returned from the food platform to the bench.

In the later experiments, and probably for some time previous to the ones just mentioned, the movement of the card displayer appeared to set up in the animals a tendency to react, and the mistakes that were made on the last day are, in my opinion, undoubtedly due to the lack of inhibition of this reflex tendency to movement rather than to mistakes in ability to discriminate.

For the next tests in this series small pieces (about one-half inch cube) of white and rye bread were presented simultaneously on a board placed outside the cage but within reach of the animal. The breads were prepared so that the taking of one resulted in a punishment and in this way an attempt was made to have the association formed quickly. The rye bread was soaked in a solution of quinine bisulphate (about 1 per cent, although no attempt was made to keep the bitterness a constant factor). The white bread contained no quinine and was presented in a comparatively moist, fresh state, but much dryer than the rye bread that had been soaked in the solution of quinine immediately before the experiments. By taking the white bread and avoiding the rye bread the animals were to show their ability to discriminate these brightnesses.[1] In these experiments and in later ones to be reported it was assumed that monkeys disliked bitter tastes; this assumption, it will be noted, proved to be true.

To obviate the objection that the smell of the quinine or the rye bread might enable the animal to differentiate the white from the rye bread, the two pieces of bread were placed in varying positions on the board. At times they were placed at equal distances from the front of the cage and within an inch of each other. In other tests one was placed directly in front of the other, and in other tests the positions were irregularly varied, one being nearer, the other farther from the cage. It might still be objected that the rye bread which was wet from having been soaked in the solution of quinine would give them a clue. That this objection was not met in the conduct of the experiments must be admitted, but the formation of the association, *i.e.*, the positive reaction would give evidence of sharp visual discrimination.

[1] In addition to its darker appearance, there was a hue difference in the rye bread. The latter, however, is the less prominent factor.

Nine animals were tested by this method, 1, 2, 3, 4, 5, 6 7, 8 and 9. This plan of tests was very gratifying in its results. It showed, apparently, complete discrimination of the two pieces of bread. The rapidity with which the habit of taking only the white bread was formed was striking. This is especially noticeable when we compare the records of the same monkeys in this and in the preceding brightness tests. As table I shows, only from 1 to 14 trials were required for each of the eight monkeys to establish the association. This is excluding the work of monkey 9, which also discriminated the white and rye bread, but which, being in the same cage with monkey 8, could have seen the latter select the white and reject the rye, and might be said to have learned from or to have imitated monkey 8.

TABLE I.

Discrimination of white and rye breads. Twenty-five experiments on each animal.

ANIMALS.	Mistakes in Trials		Total Mistakes.
	First Day.	Second Day.	
1	1, 2*a*, 4*b*	12*c*	4
2	1, 2*a*, 4*a*, 5, 6, 8	14*d*	7
3	1, 2*a*, 3, 4*b*, 6*b*, 8*b*, 12*a*	13*c*	8
4	1, 3*a*		2
5	1, 2*a*		2
6	1, 2*a*		2
7	1, 2*b*, 3*a*, 4*a*		4
8	1		1
9			

a Took rye bread after having taken white, smelled or tasted, or both, and dropped without eating.

b Took rye bread after having taken white, and ate both.

c First trial at second day; took rye bread but dropped it immediately without smelling or tasting.

d Second trial of second day; without smelling or tasting.

It is of some interest to note the individual reactions to the two stimuli that were presented. At the first trial each animal took both pieces of bread, placed them in the mouth and began to eat. In many cases the bitterness had a retarding or inhibiting effect, for the animal would take the rye bread from the mouth, look at it, smell it, and then either reject or reinsert it in the mouth.

Monkey 1, in addition to eating the rye bread on the first trial, took the piece of rye bread in the second trial, smelled, tasted and dropped it. The bitter bread was not taken on the third trial, but on the fourth the animal took first the white which it ate, then the rye, which it also ate. The rye bread was not taken on the next seven trials on that day. On the twelfth test (the first test on the second day) the animal first took the white bread and ate it, then took the rye bread and, without smelling or tasting, dropped it immediately. On this and on the following days all other tests were perfect in that only the white bread was taken and the rye bread not even handled, although, as has previously been noted, at times the rye bread was placed closer to the cage than the white bread.

Similar results were obtained with animals 2 and 3 as is indicated in the table. The rapidity of learning is remarkable in all animals, but particularly so in 4, 5, 6, 8 and 9. In each of these animals two tests were sufficient to inhibit altogether the tendency to take the darker bread. This finding is so at variance with the results of comparable tests on other animals that there would be ample opportunity to use the results as an indication in monkeys of some form of reasoning or of a marked activity to form practical judgments.

Color discrimination—white and red. The same general plan was followed as in the white and rye bread tests. Rice, cooked to such a consistency as to be stiff, was, however, used instead of bread. It was cut into small pieces of approximately the same size. Some of these pieces were used for the white stimulus, and others when colored with Congo red for the red. The red pieces were soaked in a quinine solution and used while wet. The white contained no quinine and was in a comparatively dry state. The precautions noted in the previous test were taken to prevent an animal obtaining a clue from either the relative positions or the smell of the pieces of food.

The nine monkeys which had been used in the white and rye bread experiments were tested in this experiment. The rapid learning to take the white rice and to reject the red rice, as in the white and rye bread test, was notable. From 1 to 3 trials were sufficient for the different animals to form the association. Animal 1 took the red rice only the first trial, and a similar result was obtained with monkeys 2, 4, 5, 6, 7, 8 and 9. Monkey 3 took the red rice only three times, on the first, second and fifth trials. Not only is the rapidity of the formation as striking here as in the case of the white and rye bread, but the fact that all but one of the animals formed the association, or, rather, inhibited the tendency to take the red rice, after having experienced its effect only once, may be taken as an indication of some form of reasoning.

Color discrimination—pink and green. A plan was followed in the next test similar to that employed in the white and red experiment. A smaller amount of coloring matter was used and some of the rice was colored a light pink. The pink rice also contained quinine. Another portion of rice was colored green, and this portion contained no quinine. The pink coloring was obtained, as has been said, by using a weak solution of Congo red, the green was made by using a solution of Malachite green. Both kinds of rice were used while wet, and in this experiment, it is not probable that discrimination could take place on account of difference in the light reflecting qualities of the two kinds of rice. Care was taken to have the two colors as nearly as possible of the same approximate brightness. To this end the two colors were mixed and compared to the colors pink and green of the same approximate brightnesss on a color scale and the mixed colors as well as those of the color scale were also tested by the minimal perception method in a dark room to insure their being of approximately the same relative brightness (*i.e.*, to the human eye).

In mixing the rice the coloring materials were not measured, nor was the relative amount in proportion to the water of the solutions kept constant, nor the relative proportions of the two colors to each other, nor to the amount of rice. The color solutions were usually made up anew each day and there was inevitably a greater or less variation in saturation and in brightness of the colors in different tests. These remarks hold true in all the color tests. While criticism may be made on any apparent discrimination on the basis of hue under such conditions, to the writer discrimination of color would be indicated by proper reaction from the very fact of the variation of intensities. This will be pointed out again in the discussion of the color experiments. As in the preceding tests the same control precautions were taken to prevent the animals from taking a cue either from the position or from the smell of the rice.

In this test the monkeys formed the habit of taking the green and rejecting the pink with even greater rapidity than in the white-red test. Table II gives the records of nine animals which were tested.

Color discrimination—pink and yellow. In this test the same plan was followed as in the preceding experiments. The pieces of pink rice contained quinine, while the yellow contained no quinine. As in the preceding tests, both the pink pieces and the yellow pieces were used while wet. The pink coloring was produced by the same means as in the last mentioned experiment, while the coloring of the yellow pieces of rice was produced by the addition of lead chromate. The test was controlled by the same means as in the pink-green experiment to secure the same approximate degree of brightness in the colors pink and yellow, and to prevent the animal from receiving any cue from either the smell of the two pieces of rice, or from their relative positions or relative distances. The same animals were used as in the preceding tests, monkeys 1, 2, 3, 4, 5, 6, 7, 8 and 9.

[1] The colors were mixed and compared to the colors pink, yellow, green of the same approximate brightness on a color scale, and the mixed colors on the color scale tested in a dark room for quality of brightness. In the different experiments, the pieces of rice were also placed at varying distances from the animal to prevent his taking a cue either from the smell or the positions of the two pieces.

The following is the record for the animals for 25 trials each:

No. 1	took yellow in each trial; took pink trial	1
2		9
3		none
4		none
5		1
6		none
7		none
8		none
9		none

COLOR DISCRIMINATION—PINK, YELLOW AND GREEN. The same method was employed in this experiment as in the preceding ones. In this case the three colored pieces of rice were simultaneously exposed. The pink and yellow contained quinine, the green contained no quinine. The coloring for the pink, yellow and green was the same as used in the preceding tests, each was used while wet. Exactly the same means to control the tests were employed as above. The same monkeys were tested as in the white and rye bread test, the white-red, the pink-green, and the pink-yellow experiments. Here again the rapidity of the rise of the habit of response is striking. Table III gives the record of each monkey for 25 trials.

TABLE II.

Discrimination of pink and green rice. Twenty experiments on each animal.

MONKEYS.	MISTAKES IN TRIALS.	TOTAL MISTAKES.
1	3	1
2	3	1
3	1	1
4	0	0
5	0	0
6	0	0
7	1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15, 16,	14
8	0	0

TABLE III.

Discrimination of pink, yellow and green rice. Twenty-five experiments each animal. Ten experiments first day, fifteen on second day. y, took yellow; p, took pink.

MONKEYS.	MISTAKES IN TRIALS.	TOTAL MISTAKES.
1	1 y,	1
2	1 y,	1
3	1 y, 11y	2
4	1 y	1
5	1 y, 2 y, 3 y, 4 y, 11 yp, 12 y	6
6	1 y, 2 y, 3 y, 4 y, 5 y, 6 y, 11 y, 12 y, 13 y, 14 y,	10
7	1 y, 2 y, 3 y, 4 y, 5 y, 6 y, 7 y, 8 y, 9 y, 10 y, 11 y, 12 y, 13 y, 16 y, 17 y, 18 y,	16
8	1 y, 2 y, 3 y, 4 y, 5 y, 6 y, 7 y 11 y, 12 y, 13 y,	10

We see from the records and tables that the nine animals quickly learned to select the rice that contained no quinine and to leave the quinine rice alone. The learning to respond quickly was much more rapid than, so far as I am aware, that of any other monkeys which have been studied for visual discrimination. In many cases, one trial was sufficient to inhibit any tendency to take the quinine rice, and the rapidity in inhibition of the wrong response was undoubtedly due to the use of a punishment or to the association of a disagreeable sensation, *i.e.*, to the quinine in the rice.

Another factor in the rapid formation of the habit of avoiding the quinine rice, in the case of pink in the pink-green rest, was probably the knowledge or memory of disagreeableness in red rice in the white-red test immediately preceding. In the pink-yellow test and in the pink-yellow-green test, knowledge of quinine in pink in the preceding tests, was also probably a factor in their rapid discrimination. In the pink-yellow-green test the experience of the preceding tests of pink-bitter, yellow-good, green-good helps to explain the number of mistakes in yellow rice. But, with all these allowances, we still have evidence of rapid formation of the habits of selecting one color and of rejecting another, and thereby apparently discriminating the colors red, pink, yellow and green. That discrimination has taken place cannot be doubted, but the question arises: did the animals discriminate the colors as hues, or as merely brightness values?

Watson (2), in very carefully controlled tests made on three monkeys in 1908, failed to find evidence satisfactory to him that his animals discriminated colors as hues. From later tests (3) also, on monkeys, he is still not prepared to affirm whether such discrimination is of color or of mere brightness. Yerkes (4), from the results of his well-controlled experiments on the dancing-mouse, takes a view of the matter similar to that of Watson. Both condemn the use of cards, filters, etc., in experimenting on color discrimination. They believe such methods are too loose to have much value and they urge the use of more exact methods of determining the matter.

In the experiments on color discrimination reported in this paper, as has already been stated, the criticism that the method used is a loose one may be urged. The exact proportion of the different coloring materials to the bread or rice and to the amount of water in making the solutions was not determined. Furthermore, it may be said that as the coloring solutions were mixed anew almost daily, it would hardly be possible not to have some variation in the intensities of the colors at different times. This is true, notwithstanding that means were taken to control the experiments, viz., comparison with certain color standards. Shall we infer that the experiments were thereby so vitiated as to have little or no value as a test of color discrimination? Such will doubtless be the view of some, at least on first thought, but, to the writer, these inaccuracies in method point to a different conclusion.

It is admitted that the method and apparatus used in the experiments herein reported appear loose and crude when compared to the ingenious methods and complicated apparatus used in the experiments of Watson and others. It may also be urged that my experiments take no adequate account of the question of 'monochromatic bands' in the problem of color vision. To this we may reply that in the very looseness and naturalness of the tests is to be found perhaps the strongest evidence of the discrimination of color by the animals which I tested. On the other hand I would point to the highly artificial character of the methods used by some experimenters. The complication of apparatus with its reflectors, electric shocks and other appendages is artificial in the extreme and and must result in an artificial attitude on the part of the animal. The tests, with complicated apparatus, conducted in a dark room, bring about another artificial and unusual situation, viz., the necessity for dark adaptation by the animal. The method employed in these experiments leaves to the animal a large amount of freedom and places the animal in a position as natural as is possible in such work. If, as has been urged by some, the experiments with colored cards and filters may only mean that the animals react to brightness or intensity, and not to hue relations, we should expect an animal to react to a definite relation of brightness unless we admit in animals some complex form of the feeling of relation or a certain amount of inferential reasoning. Such an explanation (*i.e.*, brightness discrimination) may be justified when the red-white test be considered alone. But how may we explain the results in the pink-green test? Half of the animals used in the latter test made no mistakes. From the beginning of this experiment pink rice was avoided, although it is not possible that the animals had any experience with red or pink rice previous to the time of these experiments. Four of the animals, therefore, reacted properly to a hue of an intensity or brightness, considered from the human standpoint, very different from that to which the animals had learned to react. Do not these positive reactions indicate rather clearly that somehow or in some way the pink rice has been taken to be equivalent to or mean some thing similar to red rice? Do not these results rather lead to the conclusion that it is not a difference in brightness or in intensity which has led to the appropriate adjustment, but rather a difference in hue? Furthermore, the fact that in the pink-green test the animals took the green and avoided the pink regardless of the difference in the amount of color is evidence that color as such was an important, and probably the only, factor in the discrimination. This view receives added weight from the similar results under similar conditions (of approximate brightness and color) in the pink-green-yellow tests.

It may, however, be urged that the carrying over of a habit from a red to a pink indicates merely a dullness in discrimination, that the red and pink may have been sensed or perceived as approximate equivalents. It must be admitted that this may be so, but the wide differences in the intensity of the red and pink would indicate rather clearly that the discrimination (or, comparison, if you will) has been due not to simple intensity relations but to hue or color similarities. For the discussion of the results in the pink-green tests, we may make two assumptions, that the two kinds of rice were of equal or unequal brightness. If we assume an equal brightness, the experiments must, it seems to me, be considered to show that discrimination has taken place because of difference in hue. If we assume the two kinds of colored rice to have been of unequal brightnesses, we must, remembering the variations in the experiments on different days, consider that the discrimination has taken place in spite of this variation. The results then lead to the conclusion that the discrimination has been due to a factor different to that of intensity on the physical side, or that of brightness on the mental. The only other factor which, in man, would produce such a reaction, is that of color.

A comparison of the results obtained in the different color tests strengthens this view. The consideration of the results in the white-red, pink-green and pink-yellow tests is instructive. Monkeys 4, 5 and 8 which had one experience in tasting bitter red rice did not take the pink rice in the succeeding tests. Now, it must be remembered that these monkeys had no previous experience with pink rice, and their avoidance of it can be accounted for only on the assumption that something in the pink rice gave a clue to the animal.

The differences (to us humans, of course) between the red and the white rice are three-fold: color, intensity, and, akin to the latter, reflecting quality or sheen. The last named was due to the differences in moisture, the red rice having been soaked in the quinine solution only a few minutes before the tests were begun. The sheen of the pink, green and yellow rice was the same, for all were equally moist at the time of the experiments. There were, therefore, only two possible differences between these colors, viz., hue and intensity.

If the avoidance of pink is to be explained, we must admit that it was due either to hue or to intensity. If it were due to a feeling of intensity difference, to an inference (taking the human standpoint, of course) that darkness means bitter and lightness means sweet we must account for a transfer from the red (very dark) to the pink (slightly dark). We must also account for the transfer in connection with the other stimuli (green) which from tests was found to have an approximate intensity equal to that of pink. So far as I can see at present, the only possible intensity explanation of this transfer is that red has a low brightness effect in comparison with other colors. However, we know that the intensities of the red and the pink rice varied greatly from each other, and we are forced to account for the phenomenon of transfer on the basis of some quality not so markedly changed. This, I believe, is the hue.

To sum up we may say that the evidence is in favor of the conclusion that monkeys have the ability to discriminate colors as such. So far as is known of the structure of the retina and of the remainder of the visual apparatus there is nothing to indicate any difference to the human mechanism, and, from this point of view, no reason exists why the monkey family may not be able to differentiate the four colors red, green, yellow and blue. The rapidity of formation of the association between the light bread and agreeableness and that between the dark bread (rye) and disagreeableness, as evidenced by their selection of the former and their avoidance of the latter, is indicative of sharp visual (brightness or intensity) discrimination. The selection of one color and the rejection of another in spite of differences in intensities speaks strongly for the discrimination of hue. The transfer of the habit from red to pink is indicative that the animals had some form of feeling of relation or of similarity between the colors as such, or that the hue (in itself and independent of intensity) acted as the appropriate stimulus to inhibit the tendency to take the particular food.

Auditory Discrimination—Noise. The apparatus used was a wooden box 22 x 18 x 10 inches and a small board or slat 18 x $3\frac{1}{2}$ x $\frac{5}{8}$ inches arranged to strike the box and thus make a noise. One end of the board or slat was fastened to the top of the box by a leather hinge. By raising the free end of the slat and suddenly letting it go, it struck the top of the box and made a sound varying in loudness with the force with which it struck. To give two sounds of different degrees of intensity or loudness two small sticks, one 3 inches in length, the other 5 inches in length, were separately used to be placed perpendicular to the box and under the free end of the board. By pressing slightly on the slat near the hinge, and suddenly removing the shorter stick, the board would strike the box and produce a noise of a noticeable intensity, and by pressing on the board as before, and withdrawing the larger stick that had been placed at the free end, the board would strike the box and produce a much louder noise. The same pressure, as nearly as possible, was exerted by the experimenter on the board in both cases. By going upon a platform arranged inside the cage when the louder noise was made, the animals were to show their discrimination of the louder and lesser noises. They were fed on the platform when the louder noise was made, and were not fed when the lesser noise was made. The noise apparatus was manipulated near the closed side of the cage in which the monkeys were kept. It was out of sight and it was not possible that the reactions were made to stimuli other than the sounds. The louder and lesser noises were made in an irregular order. Three animals, 4, 5 and 6, were tested by this method.

Monkey 4 formed the habit of responding to the louder sound and not to the lesser in eight days of 10 trials each, *i.e.*, 10 trials with each sound) or in 80 trials in all. In the first day's trials he responded to the louder sound once in the 10 trials given him, and to the lesser sound twice in the same number of trials. In 10 trials on the eighth day he always responded to the louder noise correctly and to the lesser noise only once in the same number of trials. On the ninth day he made no errors in the 10 trials given him. Monkey 6 formed the association in eleven days of 10 trials each. On the first day she responded to the louder sound three times and to the lesser four times. On the eleventh day she responded to the louder sound nine times and to the lesser sound once. She made no errors in the 10 trials for each sound on the twelfth day. In the time devoted to this work with number 5, he did not form a perfect association. In thirteen days' experiments of 10 trials each he finally came to respond to the loud sound in about 75 per cent of the trials. His inhibition of response to the lesser sound was less perfect, or about 50 per cent. Had the tests been continued it seems likely he would have become perfect in discriminating the two sounds. It is of some interest to note the apparent inferior ability of 5 in comparison with 4 and 6 in this experiment. The work of 5 appeared typical of all his work reported in this paper. Inhibition was apparently his weak point, for he responded to no matter what form of stimulus. The difference illustrates the individual variations in mental capacity of the different animals.

Sound discrimination, Pitch. Kalischer (5, p. 204 ff.) has reported experiments on sound discrimination with dogs, which animals show an ability to discrimination pitch. Although interested in the matter more from a physiological than a psychological standpoint, he incidentally obtained satisfactory evidence of discrimination of pitch by his animals. His method was to sound a certain note on an organ or harmonium as a sign that the animal should react in a certain way, such as snapping at a piece of meat. When a different note was sounded the dog was not to react and was not fed. Selionyi using a form of the 'Pawlow method' on dogs has also lately obtained evidence of discrimination by them of the tones of an organ, organ pipes and of two whistles. He also was chiefly interested in sound discrimination from a physiological standpoint.

In experiments on auditory discrimination in raccoons made by the present writer in 1906, reported by Cole (16, p. 230) evidence of pitch discrimination by those animals was obtained. In the experiments on pitch to be reported in this paper I used the same plan that I had employed with the raccoons, and which is similar to the method Kalischer has used. An ordinary German mouth harp or harmonica A was used. When I sounded the higher note, A 3, the monkey was to go upon the platform used in the preceding noise tests, and was fed there when the note was sounded. When the lowest note, A 1 (two octaves lower), was sounded he was not to go up and was not fed. The notes were sounded in an irregular order so the animal might not react in a rhythm to the sounds. Care was taken to sound the notes with the same degree of intensity, as nearly as possible. I took the usual precautions that the animal should not obtain a cue from my looks, motions or in any other manner, and react to these stimuli rather than the tones.

The records for the different animals are as follows:

Monkey 4 formed the habit of responding to the high note, A 3, and not responding to the low note, A 1, in three days tests of 10 trials each.[1] On the first day monkey 4 responded to the high note six times in 10 trials, and not at all to the low note. On the third day he responded to the high note ten times and to the low note three times. On the fourth day of the experiment (three days after the experiments just noted) he responded to the high note ten times in 10 trials and to the low note not at all in the same number of trials. When tested on the fifth day, two days later, he was perfect in 10 trials.

Monkey 6 formed the association in four days, in 40 trials in all. On the first day she responded to the high note twice in ten trials and to the low note not at all. On the fourth day she responded to the high note nine times and to the

[1] *i.e.*, ten trials of the high note and ten trials of the low note.

low note once in 10 trials of each note. In the 10 trials on the fifth day she was perfect.

Monkey 5 did not form the habit of correct response in six days' tests of 10 trials each. In the 10 trials of high and low pitch on the sixth day he responded to the high note six times and to the low note only once.

If we may generalize from the work of the animals tested in the above experiments we may conclude that Rhesus monkeys discriminate quantitative differences in noises. They also discriminate musical notes of widely different pitch. These experiments also indicate that monkeys learn to discriminate pitch with considerably more facility than do raccoons in similar tests.

Inhibition of habit.

The activities or modes of behavior of animals, including man, are of five kinds: (*a*) reflex actions; (*b*) instinctive actions; (*c*) habitual actions; (*d*) intelligent actions; (*e*) rational actions. These different actions, in varying degrees, characterize the different orders of animals. Generally speaking, the lower the order of the animal the more the *a* form of action is present, and the higher the order of the animal the more the *d* and *e* forms of action are present. The lower forms are, however, basal for man as well as for the amoeba.

Habitual actions are seen not only in the behavior of animals of the higher orders but also in the activities of man. Many apparently intelligent actions of man are of the nature of habit. To say that habit dominates man's actions to a very large degree, and reason to a very small degree is only to state the simple truth.

Previous observations have indicated to comparative psychologists that monkeys and other mammals have a marked tendency to form habits of action. This was well shown by Lloyd Morgan and many others. A certain perceived stimulus is followed by an agreeable or by a disagreeable result, and an animal soon forms the habit of reacting in the appropriate manner when the stimuli are given. The formation of habits is, however, only one way of meeting the conditions of the environment. Habits once acquired often become useless, and sometimes have to be replaced by actions opposite in character. In other words the inhibition or the replacement of a habit is often necessary to the well being of an animal or of man, and the ability to inhibit definite modes of reaction, including reflex tendencies, is important for advancement.

To test in some measure the ability of monkeys to inhibit a recently formed habit four experiments were made. Three monkeys which had learned to discriminate white (normal) from rye (quinine) bread were reviewed on this work daily for seven days in order that the habit be firmly established. A rest period of seven days was given, and then the memory tested. The results of the latter test showed a perfect retention by all the animals. On the succeeding day each monkey was presented with some pieces of rye bread that had not been soaked in quinine. Then an experiment was performed in which the rye bread was not made bitter. These tests were continued for seven days, 10 tests on each day. The results are given in Table IV. It will be noticed that on the first day monkeys 6 and 8 disregarded the rye bread, and monkey 8 continued to disregard it for two more days. Monkey 7 on the fourth trial on the first day took the white bread first and then the rye. Five times thereafter she repeated this, and the habit of refusing the rye bread when it was simultaneously exposed with the white was broken. Thereafter, on the six succeeding days of the experiment she left the rye bread only once.

After this series a second test was made in which the white bread was made bitter with quinine. Both pieces of bread were presented simultaneously on a board, as in the previous experiments. The results of the tests, 10 experiments a day for seven days, are given in Table V. It will be seen that the animals .soon learned to avoid the bitter bread. The mistake made by monkey 6 on the second day was only a partial mistake for the animal picked up the white bread, smelled it and immediately dropped it. The inhibition in the case of monkey 7 was fixed on the first day after four mistakes had been made.

A third series was then made in which the small pieces of bread (white, bitter; rye, normal) were presented successively. In these experiments the animal had a choice of taking or of leaving the single piece, white or rye. The results are given in Table VI. It will be seen that the three animals avoided the white bread on the first day of the experiments, but that on the second all took the white at least once. On the second day monkey 6 took the white bread six times, and monkey 8 four times. After these experiments both animals disregarded the white bread when it was presented to them on the board, even though they were very hungry. Only once during the remainder of these tests did monkey 6 touch the white bread. On the fourth day, when the white bread was presented, she put her hand through the wire of the cage and swept the piece off the board.

The fourth series of experiments was begun after the seven days of series 3. In this last series the breads were presented to the animal by the experimenter, to see if the inhibition effect, or the new habit of leaving the white bread was associated with the presentation of the bread upon the board outside the cage. Each piece of bread was taken by the experimenter in his fingers and held just within the wire netting of the cage. Even with this added inducement monkey 6 refused to take the white bread, but both of the other animals finally, and monkey 7 repeatedly, took it. In the 70 trials with monkey 7, she took the white bread fourteen times. At the sixth trial on the fifth day monkey 7 struck at me when I offered her the white bread, as if angry that it should be presented to her. Monkey 8 took the white bread twice on the second day, but after he had smelled it he dropped it. On the fifth day he once reached toward the white bread that was offered him, but drew back before he had touched it. On the sixth day he struck at the white bread or at me just as he had monkey 7 on the previous day.

These experiments show, on the part of the monkeys investigated, a rapid inhibition of a previously formed habit, and a rapid acquisition of a habit opposed to the original habit. The acquisition of the new habit, however, took a much longer time than that of the original habit, as can be seen from a comparison of the results. In the later experiments so many new factors, especially those of tempting the animal by single presentations, were introduced that the curves of learning cannot be directly compared. It would be fair, however, to compare the results in Table V with those in Table I. When this is done it is seen that the replacement of the association is almost, if not equally, as rapid as the formation. Should this result be true for other activities of the monkey, it would indicate a rather high degree of adaptivity, which goes far towards the production of apparently intelligent actions.

TABLE IV.

Inhibition of habits. All monkeys had learned to avoid quinine (rye) bread; no quinine in either bread for this test. W = took white, R = took rye. Ten trials each animal daily.

MONKEYS AND SERIAL DAYS.	1	2	3	4	5	6	7
6	W 10	W 8	W 10	W 10	W 10	W 10	W 10
	R 0	R 8	R 10	R 10	R 10	R 10	R 10
7	W 10	W 10	W 10	W 10	W 10	W 10	W 10
	R 5	R 10	R 10	R 9	R 10	R 10	R 10
8	W 10	W 10	W 10	W 10	W 10	W 10	W 10
	R 0	R 0	R 0	R 4	R 8	R 10	R 10

TABLE V.

Inhibition of habits. All monkeys had been practised on preceding tests (Table IV). Quinine in white bread, rye bread in natural state. Simultaneous presentation. R took rye, W = took white bread. Ten trials daily.

MONKEYS AND SERIAL DAYS.	1	2	3	4	5	6	7
	W 1	W 1*	W 1	W 1	W 10*	W 10	W 10
6	R 10	R 10*	R 10	R 10	R 10*	R 10	R 10
	W 4	W 0	W 0	W 0	W 0	W 0	W 0
7	R 10	R 10	R 10	R 10	R 10	R 10	R 10
	W 3	W 1	W 0	W 0	W 0	W 0	W 0
8	R 10	R 10	R 10	R 10	R 10	R 10	R 10

TABLE VI.

Inhibition of Habits. All animals had been previously practised on experiments recorded in Tables IV and V. Quinine in white bread, rye bread in natural state. Successive presentations. W = took white, R = rook rye bread. Ten times daily.

MONKEYS AND SERIAL DAYS.	1	2	3	4	5	6	7
	W 1	W 6	W 0	W 0	W 0	W 0	W 0
6	R 10	R 10	R 10	R 10	R 10	R 10	R 10
	W 0	W 1	W 1	W 5	W 1	W 1	W 2
7	R 10	R 10	R 10	R 10	R 10	R 10	R 10
	W 0	W 4	W 0	W 0	W 0	W 0	W 0
8	R 10	R 10	R 10	R 10	R 10	R 10	R 10

TABLE VII.

Inhibition of Habits. All animals had been previously practised on experiments recorded in Tables IV, V, and VI. Quinine in white bread, rye bread in natural state. Successive presentations through wire netting of cage instead of outside. W = took white, R = took rye bread. Ten trials daily.

MONKEYS AND SERIAL DAYS.	1	2	3	4	5	6	7
	W 0	W 0	W 0	W 0	W 0	W 0	W 0
6	R 10	R 10	R 10	R 10	R 10	R 10	R 10
	W 0	W 0	W 3	W 5	W 2	W 3	W 4
7	R 10	R 10	R 10	R 10	R 10	R 10	R 10
	W 0	W 2	W 1	W 0	W 1	W 0	W 0
8	R 10	R 10	R 10	R 10	R 10	R 10	R 10

Imitation.

Whether or not the higher animals learn by imitation is a mooted question among comparative psychologists. Involving as it does the presence of ideation or of 'transferred association' in the animal mind, it is of the utmost importance that this mental function be carefully considered. Most experiments in the field of comparative psychology are inclined to deny the higher forms of imitation to animals. Morgan (7), Thorndike (8, 9) Yerkes (4), and Watson (10), take this ground, while Kinnaman (11), Hobhouse (12), Berry (13), Haggerty (14) and others believe they have found evidence of imitation, apart from mimicry which is sometimes spoken of as imitation and which may be considered a reflex adaptation to certain stimuli.

It is necessary first to define precisely the term imitation as it is applied in comparative psychology. The term imitation is used in a number of different senses. First it is used as a synonym for what may be more accurately called instinctive imitation. This is the form of imitation shown by all animals in the performance of certain necessary acts, such as that of a chick when it pecks at a bit of food on seeing another chick do so. A second use of the word is in relation to mass activities and we may therefore speak of it as gregarious imitation. This form of imitation is the performance of an act similar to that performed by another animal of a herd or flock, the latter act being due to a definite stimulus but the act of the imitator, although similar to that of the imitatee, is not produced by this same stimulus but by the stimulus of seeing or of hearing the first animal. This form of imitation is illustrated by a herd of buffaloes running off in alarm or a flock of sheep following their leader and jumping over an imaginary obstacle. Lastly, there is inferential or reflective imitation, where one individual sees another perform an act, realizes or understands the consequences of that act, and thereupon performs a similar act with the idea of getting the same results.

Of the above forms of imitation the first two are of a low order, and perhaps no comparative psychologist will deny them to many of the higher animals below man. The controversy, however, arises regarding the presence and the amount of the higher form of imitation, the imitation that has been defined as inferential. An attempt was made to test the presence of this form of imitation, and three separate kinds of tests were made.

An apparatus and plan similar to those already employed by Hobhouse (12, chap. x), and by Watson (10, p. 175), with which the former obtained positive and the latter negative results, were used by me in this work. This apparatus consists of a glass tube or cylinder, 15 inches long and $\frac{7}{8}$-inch in diameter, and a plunger of wood less in diameter but

3 inches longer than the tube. The diameter of the wooden plunger was such that it could be readily inserted in the tube and was freely movable. A piece of food, usually banana or a peanut, placed crosswise in the tube, was inserted in the tube and pushed down about half way. A piece of food was always selected sufficiently large not to drop through the tube when the latter was held in a vertical position, and yet sufficiently small that it could be readily displaced when the plunger was used to push it through the tube. This apparatus was presented in turn to eight monkeys and the imitation tests were begun after each had failed to manipulate it three times. The tube and the stick were placed in the cage with the animal to see if he could push out the food after his attention had been attracted and he had been shown by the experimenter how the stick was to be manipulated for obtaining the banana or peanut. I endeavored to have the monkeys see me perform the act at least three times before each trial, and to this end I watched carefully each animal while the exhibition was being given. If there was in my mind any doubt that the animal had paid close attention to the performance of the act by me, I repeated it. Following the three demonstrations the animal was given the apparatus for one minute, and this amount of time was allowed him in which to perform the act. In the case of some animals these tests were made daily and in the case of others there were intervals of one or two days. The variation in the time routine did not appear to have any influence upon the learning. The number of trials for each animal was also variable, from 24 to 96, each having seen the act performed three times before each trial, *i.e.*, having seen the act from 72 to 288 times.

All the monkeys failed to show any signs of imitation, and I was unable to verify on these animals results like those reported by Hobhouse. At the end of the tests all animals acted in much the same way as at the beginning of the tests and the first notes may be used to indicate the general character of their activity. They bit at the food in the tube, looked into the end of the tube at the banana, jerked the tube around, often took up the stick and bit it; sometimes picked up the stick and threw it away, dropped the tube, as if they gave it up. While I was exhibiting the mechanism an animal would watch the operation closely, and at the moment the food appeared on the end of the tube within the cage, he seized it. Sometimes they attempted to reach into the tube to get the food. In this experiment I also tried Hobhouse's plan of 'suggestion.' When an animal was wrestling with the tube I pointed to and moved the stick towards him to call his attention to it. Thereupon, sometimes, an animal took up the stick, but only to throw it away after a few seconds. In brief, throughout the three weeks of this experiment not one of the animals appeared to show any understanding of the problem, and any ability to cope with it. It may be urged that a greater number of trials than 24, the number some of the animals received, would have brought success for some of them, but, were imitation of this kind an important and a constant condition in monkeys, this number of trials would have been sufficient for them to demonstrate its presence.

A second experiment on imitation was then performed. This experiment was also similar to one made on monkeys by Hobhouse (12, chap. x) and by Watson (10, pp. 173, 174) A T-rake, consisting of a light handle 18 inches long, with a cross piece 4 inches long nailed at one end, and at right angle to the handle, was used. This was placed with the handle end through the wire side of the cage and with the T-end extending outward on the floor of the room. A piece of banana or a peanut was placed on the floor outside the cage but within the sweep of the rake when handled by an animal inside the cage. In this experiment I wished to see if the monkeys could grasp the situation of the use of the rake. Two separate factors of adjustment enter into this; first, the use of the rake for the use of grasping or reaching out towards the food, and, secondly, the use of the implement to draw the food nearer the cage and within reach of the hand itself.

Monkeys 1, 2, 3, 4, 5, 6, 7 and 8 were tested in this manner.

I showed an animal, by hooking the T around the food and pushing the rake forward, but necessarily from the outside of the cage, three times before each trial. I then let the animal try the apparatus for two minutes. Each of the eight animals was given from 24 to 120 trials in the four weeks these experiments were continued. Each animal had failed to use the rake three times, without being shown, before the imitation tests were begun.

Monkeys 1, 2, 4, 5, 6, 8, failed as completely as in the previous experiment. Each seized the end of the rake handle within the cage, jerked it around, bit it, tried to reach the food with their hands through the wire of the cage. Sometimes they became angry and threw the rake aside; in some cases they finally gave it up. I noted, however, that all soon came to pull the rake into the cage, up to the T, which was as far as the wire of the cage would permit. So far as I could tell, each animal watched me closely while I was showing them, appeared to have much interest in the proceedings, and seized the food through the wire of the cage the moment it was within reach. But what the six monkeys did *not do* was to push the rake out and hook the cross piece around the food. In all of the trials with these six monkeys so far as I could see, none gave a sign of using the rake as a tool with which to draw the food toward the cage. Here again it may be urged that a greater number of trials than 24 should have been given to all animals, but the simplicity of the apparatus was such that in a child of the same age one or two trials are sufficient for a successful imitation.

After several days tests with two of the animals, 3 and 7, there appeared some evidence of imitation. Monkey 3 learned to *push out the rake*, and, with much slashing about of the T end, would draw in the food. The securing of the food by hooking the T around it was, however, very awkward work for her. Sometimes she would knock the peanut away in her effort to pull it in. On its being replaced by me she usually succeeded in drawing it in roughly. The best that can be said of her performance is that she appeared to learn to imitate me perfectly in pushing the rake out, and awkwardly in pulling the food in with the rake.

Following is an account of the tests on the third day. It will be remembered that the method of securing the food by the use of the rake was demonstrated three times before each test. On this day, however, this demonstration was omitted after the seventh test. Trial 1 was a failure; the animal did not attempt to take hold of the rake after being shown three times. On the second trial, however, after the three demonstrations, the animal grasped the rake, and moved it about in the direction of the food. She succeeded in bringing in the nut sufficiently far so that she reached it and ate it; time, 85 seconds. In this first successful attempt she grasped the rake awkwardly and did not at first seem able to manage it, but eventually she managed to swing it around so that the nut was caught and, as has been said, pulled in the nut so that she secured it by reaching through the bars of the cage. In the next trial the movements of handling the rake were about the same as those in the preceding trial, although she had acquired some facility in the use of the tool, and managed to secure the nut in 29 seconds. On the fourth trial, during the demonstration she took hold of the rake and pushed it outwards toward the food. After this demonstration she pushed the rake outwards in an apparently purposeful manner and secured the nut in 20 seconds. The fifth trial was similar to the fourth, but with an increasing ease in adjustment and with apparent greater facility in the use of the rake. In 8 final trials on this day the demonstrations were omitted but the actions of the animal were similar to those mentioned in connection with the fourth and fifth trials. On the sixth and tenth trials the manipulation of the rake caused the nut to roll further away from the cage and each time I replaced it. The times for these 8 trials were respectively: 15, 15, 9, 12, 24, 14, 4 and 8 seconds.

Three days later, the fourth day of the tests, demonstrations were given in the first and second tests. The results, however, were similar to those on the third day, but with shortened times for the performance of the act. The times were as follows: 10, 8, 3, 4, 2, 6, 7, 9, 5, 4, 3, 9, 2, and 2 seconds. On the seventh trial she managed to get the nut only part way with the first pulling movement, but then, with apparent intention, she gave another pull and brought the nut within reach. On the thirteenth trial the nut rolled away when touched with the rake but the animal pushed the rake out farther beyond the nut and managed to secure it in the usual way.

Monkey 7 had failed to show evidence of imitation on six days of 3 trials each when I manipulated the rake and attempted to exhibit the mechanism of securing food. It was thought that the operation of another animal might be imitated, and for this reason I placed her with monkey 3, which at that time had learned to use the rake with facility. In these experiments I carefully watched monkey 7 in order to be reasonably certain that she observed monkey 3 perform the act with the rake. Only after I was sure the animal had been looking in the direction of monkey 3 while the latter performed the act three times did I begin a test of monkey 7. At these times also I called attention to the tool by pointing to it and to the food. In each trial she was permitted two minutes to perform the imitation act, and if it was not performed in that time I counted the test a failure. The first five tests were failures, and likewise the seventh and tenth. On the sixth trial she pushed the rake outwards awkardly, but with such movements that she could not secure the food. Finally, however, after 79 seconds, she managed to hook the cross pieces about the food and immediately pulled it in. The eighth and ninth trials were similar to the sixth but the food was secured in 20 and 8 seconds respectively. The tenth trial I have counted a failure, for in this case the animal pushed out the rake and pulled it in but did not manage to get the cross piece hooked around the food. In the two minutes allowed for the performance of the act, she did not manage to secure the food.

On the second day of these tests she failed to show by her actions any evidence of imitation in the second, third, sixth, ninth and tenth trials. Only ten seconds were required for the proper performance of the act on the first trial, for she immediately manipulated the rake so that it caught the nut and she pulled it inwards. The fourth, fifth and seventh trials were similar, 12, 15 and 9 seconds respectively. Although the eighth trial was counted a failure the monkey did manipulate the rake properly, but in pushing it outwards the nut was hit and rolled farther way. In the final trials of this day the animal did not make many efforts to use the rake, although two minutes were allowed. In these experiments the movements of pushing the rake outwards were well performed, but those of pulling inwards were very badly executed. This was also noted on the following day, when in 6 trials, she managed to secure the food four times, in 17, 9, 11 and 5 seconds. In subsequent trials she continued to be incoördinate in the pulling in movements, but accurate in pushing out the rake.

It may be said that monkey 7 did not make the mental connection between seeing me manipulate the rake and the idea of the acquisition of food, but that some connection or association was formed between seeing monkey 3 perform the act and such an idea (or what corresponds to an idea in the monkey mind). The impulse to handle the rake, to manipulate it and to use it in connection with food may properly be said to be due to her having seen monkey 3 obtain food by the use of this tool. This impulse or association may be explained as A, ideal, or B, imitative. In the present state of comparative psychology it is best to consider it the latter, and we conclude that the use of the rake by monkey 7 has been due to an imitative impulse, the tendency to perform the same or a similar act performed by another.

In a third experiment to test the ability to imitate the apparatus used in a previous test of analogical reasoning was again employed. A piece of banana was suspended from a pole that extended across the room. The food was placed about 4 feet from the floor, high enough to be beyond the reach of the animal when it stood upright on the floor. A light pole, 9 feet long, 1.5 inches in diameter, was loosely attached at one end by means of a pivot to a support and extended 7 feet across the open space of the room to a horizontal supporting board (*C*), which was 2.5 feet from the floor. Figure 2 shows the arrangement. On the horizontal board the free end of the pole was easily moved, and by shifting it to the position *P-X* the pole was brought under the suspended food. To bring about this change it was necessary to move the free end of the pole about 3 feet, with corresponding decreases in the amount of movement the nearer to the pivot the pole was grasped. In the position *P-X* the animal was able to reach the food if it climbed upon the pole. The problem for the animal was to slide the free end of the pole sufficiently far on the horizontal board or

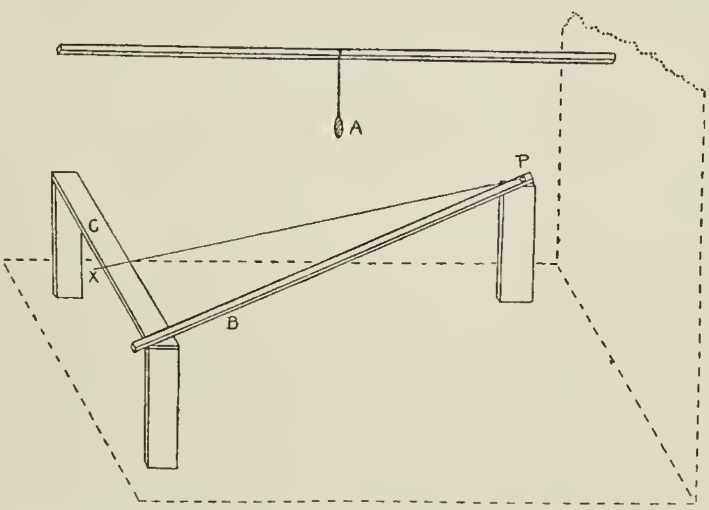

Fig. 2. Diagram of apparatus used in tests of reason and of imitation. *A*, suspended banana; *B*, sliding pole in original position; *PX*, position to which pole had to be moved in order that the animal might reach the food; *C*, supporting slide.

slide, to bring the pole under the suspended food: to go upon the pole and get the food by reaching upwards about 10 inches.

Monkey 3 had been tested with this apparatus in a series of experiments on analogical reasoning (see p. 52 ff.) and had failed to manipulate it. In this first test she was not shown how to manipulate it, but was shown when the experiments on imitation were begun. Before each test by the animal I moved the pole from its original position to that of *P-X* and permitted the animal to obtain the food each time. After having been shown in these three trials that the moving of the pole was essential for the securing of food I allowed two minutes in which the animal might perform the act. If the act was not accomplished in that time I called the test a failure and repeated the demonstrations. On the first day of this experiment she succeeded in performing the complex adjustment of pole, etc., four out of eight times. On the first trial she moved the pole awkwardly, but sufficiently far that she was able to reach the food; 30 seconds. On the second trial she moved the pole only a trifle at first, but returned to it and succeeded in moving it the required distance; 25 seconds. On the next trial she changed the movement of pushing to that of lifting the pole and pushing at the same time. In this her movements were awkward but she managed to get the food in 45 seconds. The next three trials were counted as failures, although in all of them the animal manipulated the pole and showed signs of recognition of the use to which the pole was to be put. On the fourth trial after having moved the pole a short distance and not obtaining food, she tried to get the food by going to the cross piece from which the food was suspended. On the fifth trial she pushed the pole off the slide, and could not get the food. On the sixth trial she moved the pole a short distance, and refused to complete the act. On the seventh trial she moved the pole sufficiently far to obtain the food, 20 seconds; but on the eighth trial she did not try to move the pole or to obtain the food.

On the following day she was not shown how to manipulate the apparatus the first three trials, two of which were successful, but was shown on the other seven trials. At first she pulled the pole part way, but it appeared to be difficult work, and she obtained the food in 20 seconds. The second trial she pulled the pole about 6 inches and stopped; I took hold of the pole and helped her to move it a few inches, and then she made a great effort and pulled the pole sufficiently far to obtain the food. The third trial she did not attempt to move the pole. The following seven trials were, it has been mentioned, preceded by three demonstrations each. In only the fourth trial did she take hold of the pole, at other times she did not attempt any manipulation. On the third day of the tests, six days following that just mentioned, she failed to manipulate the apparatus in six trials, although she was shown three times before each trial. Only once on this day did she touch the poll. Two days later, she failed on the first six trials. On the seventh she helped me to push the pole when I was demonstrating it, and following this she managed to get the pole moved by her own efforts in 68 seconds. On the eighth trial she moved the pole part way and obtained the food in 30 seconds. The ninth trial was a failure in two minutes. Seven days later she was given three trials and was not shown how to manipulate the apparatus. She did not exhibit any signs of ability to manipulate the apparatus and was then shown. The fourth trial was a failure; on the fifth, she took hold of the pole at the time I was moving it, and after the demonstrations she immediately attempted to move the pole, which she managed in 36 seconds. On the sixth trial she managed to get the food in 26 seconds. The seventh trial, 18 seconds, and the eighth trial in 9 seconds were successful. In the remaining 17 trials on this day she gradually lowered the time for the performance of the act although her actions were always about the same. Two days later there were no failures in 20 trials, but the actions of the animal in 2 of the trials were noteworthy. In these trials the monkey moved the pole part way, then stopped, and appeared to be observing the amount of the movement and making a judgment regarding the possibility of reaching the food from the pole in the position in which she had placed it. Then, not liking the position she moved the pole farther and climbed upon it and obtained the food.

In these tests there was a gradual learning to eliminate unnecessary movements and to perform the necessary movements in a satisfactory manner. The experiment does not, however, wholly belong to the type of learning of trial and error, for, as has been remarked, the monkey moved the pole properly the first time she attempted to do so. There was no previous groping for a something, no fumbling with anything but the pole. The attention, so far as shown in action was directed to the pole. There was an immediate grasp of the situation, and this coming after she had been shown that the food was to be reached by changing the position of the pole is taken as evidence of imitation. The fact that in later tests she failed to pay attention to the pole and to attempt to move it does not mean that she could not imitate, but it is well known that the monkey is extremely variable in its actions, and its attention is held with difficulty. We know that in the production of habit, as many observers have pointed out, there is not a gradual shortening of the time of reaction as in the case of cats and dogs, but that the time for the performance of an act is extremely variable. It is this factor of variability due to elements such as lack of hunger, etc., which seems to me to account for the lack of attention to the problem. It is further to be noted that in the successful trials she used a variety of means for the accomplishment of the end; sometimes she pushed the pole unhesitatingly through the required distance; at times she pulled it, and at times she pulled and pushed it. The variety of action indicates that the performance of the act is not like that of a habit formation, but that of attempting to accomplish an object in any manner that this could be done. The performance of the act was awkward, but there appeared to be imitation of a relatively high order.

In the experiments that have been described evidence of imitation by monkeys 3 and 7 was found in the second experiment of the series. These two monkeys and the other six failed to show signs of imitation in the first experiment and I failed to find any signs of imitation on the part of the six monkeys in the second experiment. When these results are combined, I think they indicate in a general way that some monkeys may and do learn by imitation. The amount of imitation is not shown, but the fact that in so many simple experiments negative results were obtained indicates that these animals do not imitate to the extent that has been ascribed to them. We are, I believe, justified in concluding that imitation is a mental function of the monkey or of some monkeys, although the results of my experiments may be interpreted to mean that imitation does not play a very important part in their learning process.

Ideation.

The evolution and anecdote schools attribute ideation of a comparatively high order to the animals with brains similar to that of man, and they hold that ideas similar to those of man play an important part in animal behavior. They base this belief, however, upon uncontrolled observation and upon flimsy and circumstantial evidence and not upon controlled experiments and unequivocal facts. The evolutionists also appear to believe it necessary to attribute ideas to animals, else the doctrines of a progressive mental development would not coincide with that of the physical development. On the other hand, others, mainly experimenters, hold that the casual observations do not indicate animal ideation and the same facts are interpreted differently. Special tests have given negative results and from these and from the careful observation of animals in laboratory surroundings they conclude that animal ideation is not proven, and that the present evidence tends to indicate an absence or a lack of ideas in the animal mind. Some, however, are willing to admit that animals may have ideas, although in small number, but are forced to conclude that ideas are a very unimportant element in minds below that of man. It is true that for the most part the experiments of these men have not been special tests for ideation, but tests for other reactions in which ideation, if present, should or might have been exhibited.

Before passing to the evidence of ideation, it seems well to define the term 'idea' and thus to have a precise notion of what we should look for in animal behavior. For the purpose of comparative psychology an idea may be defined in the following ways: *a*, an image or picture of a visual object which is formed by the mind; *b*, a general notion or conception; *c*, a plan or purpose of action or an intention; *d*, idea in the sense of an understanding of a certain relation or situation (as of sensible objects). If, therefore, an animal exhibits any of the mental conditions noted above we may conclude that ideation is present.

A simple test for ideation was made with animals 1, 2 and 3. A board 20 inches long and $3\frac{1}{2}$-inches wide was placed with one end against the side of the cage and within reach of the animal with the other end extending outwards from the cage and beyond reach. Food was placed at the farther end. The object of the tests was to determine if an animal would understand the situation and be able to secure the food by the indirect method of pulling at the board near the cage rather than by directing its efforts at the food or the food end of the board which was beyond reach. In the first trial monkey 3 after some testing of the openings of the wire netting, seized the board awkwardly near the end within reach, pulled the board alongside the cage and secured the food, 30 seconds. In this trial the attention for only a few seconds was directed to the food position but was directed mainly to the end of the board that did not contain the food. The actions of the animals in manipulating the board were awkward but only in the method of reaching for and handling it. The actions were directed to the board, and not to the food, and it appears that here is an example of an understanding of a situation, a direction of action to an end not of special interest in the situation. In the second and third performances she duplicated her actions, but with shorter times for its accomplishment because the awkwardness largely disappeared and because the preliminary direction of attention to the food was absent, 20 and 10 seconds. In later tests she continued to react with complete success, reducing the time to three seconds in the fifth trial, which time includes that for the manipulation of the board and the securing of the food. Monkey 1 succeeded in the third trial but showed more misdirected efforts than did monkey 3. The third animal also managed to perform the necessary acts to secure the food in the third trial, but the actions were more awkward and poorly directed than those of monkeys 1 and 3.

At the beginning of this test there were many unnecessary movements on the part of each animal, but this awkwardness or lack of understanding of how to deal with the situation was mainly that of motor adjustment. It was a difficulty in dealing with the board through the wire netting of the cage and not a difficulty or a lack of understanding of the problem, viz., of reaching for and of manipulating the board. In the first trial of monkey 3 the animal directed its attention to the board near the cage, and showed that it appreciated the relation between this part of the board and the securing of food. The later reduction in time for the performance of the act, which at first sight makes the experiment appear to be only another instance of learning by trial and error, was mainly that of a proper adjustment of arms and hands to the wire netting and not that of attacking the board at the proper place.

These results indicate the presence of ideas of the above described third or fourth class, *i.e.*, a plan of action or an understanding of a situation. It was apparent that from the first the animal understood that the food could not be reached directly but must be obtained by an indirect attack on some other part of the apparatus. There was in this case no general activity such as has been described by numerous investigators in connection with the puzzle box experiments; the trying here and the pulling there were notably absent, and the attention of the animal was concentrated on the board and on the getting the food. For a few seconds after the board and food were displayed the hungry animal would reach in vain for the food which was beyond its reach. but this unsuccessful method was given up and the attack directed to the board on which the food was placed. It is also of great importance to note that the attack was directed not to the part of the board on which the food was placed, but to the part of the board away from the food.

The simplicity of the situation may be urged as an argument against the presence of ideation in solving the problem, but I would again call attention to the ordinarily complex character of similar experiments that have given negative results to other observers. In such experiments we must steer clear of both an absolute simplicity and of a complexity abnormal to the animal. We must test the animal under conditions which are within its mental range and which will show the ability to reason. It is equally bad to set impossible conditions and to draw unsupported conclusions, and in both these ways comparative psychologists have attempted to solve the problem of animal reason and ideation. To conclude, for example, that a dog or a monkey possesses no power of ideation or of reasoning because it does not thread a needle when the needle and thread are supplied or because it does not unlock a door when the key is presented is to limit the terms ideation and reason to the ability to perform certain activities connected with a certain class of civilized man. The examples cited are, perhaps, extremes but they illustrate the attitude of a certain class of experimentalists. On the other hand, to conclude that a cat reasons because it attracts attention by scratching on the window pane or because it manages to strike an electric button for the opening of the door is to take no account of the possibility of previous training of the nature of trial and error. We should steer between the two extremes and test the so-called higher powers of animals by presenting to them conditions appropriate to the class. In the simple experiment described in this section and in others to be described in the section on reasoning (p. 52 ff.) the conditions are appropriate to the monkey family. We have presented to the animal a new problem which it must solve, a set of conditions to be dealt with in order that a resultant pleasure (hunger satisfaction) ensue. In the solution of the problem the animals took a direct path. There was none of the fumbling or groping, no trying here and there, no attempts upon other parts of the cage or its surroundings, but a direct attack upon the board that held the food.

Somewhat similar actions were observed and noted above in connection with the food box experiments. After the previously ignorant (so far as the food box is concerned) monkeys had been fed from the food box a few times so they might become acquainted with the location of the food, the doors were closed. Then began an attack upon the doors, not upon the wire netting of the sides of the cage. The animals remained active about the doors of the food boxes;

they did not sit sulkily upon the platform in the cage; the attention was directed to the doors of the food boxes. Thorndike indicates a belief that the attention of his cats was directed to the string or to the button because the movement in connection with these parts of the appliances was followed by a pleasure. With my monkeys, however, the attention of the animals could not have been directed to the doors of the food boxes on account of a pleasure in connection with a movement, for the animals did not have to perform any movements (except those of taking the food from the boxes) in the neighborhood of the food boxes, and in connection with the doors there were no movements to be performed. In these cases there was no activity in the region of the food boxes or of the doors which could become associated with a pleasure. The attention must then be due to something different from the movement-pleasure association; and it seems probable that there is some form of ideation to account for it.

In the brightness tests interesting behavior on the part of monkey 3 was observed. As has been explained above, the doors were opened by turning a button and thus food was secured. After several days' experiments monkey 3 began to close the door of her own accord instead of waiting for me to close it preparatory to another trial. Before I could close the door after she had opened it she would close it. This would expedite the getting of food. It soon became almost an habitual custom for her to close the door, and in the remaining tests she did so from three to ten times each day throughout the experiments. Why should an animal close the door if it was not with some design or intention (perhaps vague) of hurrying the food-getting process? It seems too parsimonious and even inadequate to call this kind of reaction a mere reflex. In the puzzle box experiment the same animal and two others, monkeys 1 and 2, often attempted to hold open the door with their hands and this kind of action was recorded many times throughout the series. At first I was inclined to consider this action as a reflex but it is overworking the meaning of the term reflex to use it to explain or to describe the actions of the three animals in this particular. Somewhat similar behavior in a different situation was also noted with monkey 8. In testing his color discrimination with colored rice and breads, the food was placed on a block about a foot away from the cage and then moved close to the cage so that he could readily reach the food through the wire netting. When the food was taken or rejected the block was moved backwards a short distance preparatory to placing food upon it for another trial. During the first few experiments he acted as most of the monkeys do, viz., moved away from the side of the cage as soon as the food was obtained. After he had learned to discriminate the colors, however, it was noted that he began to push the block away after having taken the appropriate food. This he continued to do during the remaining trials on that day. The actions of monkey 8 in this case had all the appearances of an understanding of the situation and a desire to hurry the getting of food. It is of considerable importance in this connection to bear in mind that the performance of this action was sudden and not a gradual growth and not the result of trial and error.

The actions of the three animals in the situation of the diagonally placed board, the actions of monkey 3 in closing the door of the food box and the actions of monkey 8 in pushing away the block on which food was placed have a similar appearance. Were we not obsessed by the law of parsimony we would immediately say that these actions show the presence of ideas in these animals, ideas of the highest form. We are, however, in keeping with the law of parsimony, quite justified in saying that the actions indicate the presence in the minds of the animals of a something very much like an idea in the human sense, a something that has for the animal a function or a use similar to an idea in man. This 'something' may be crude and simple, and doubtless it is analyzed by the animal, but it serves practical purposes. These somethings may be termed, as does Hobhouse, practical ideas; they may be partly sensory-motor reactions, and may be partly instinctive, and in part they may be accounted for by the superior equipment of the monkey in coördinated vision and in the use of the hands. The evidence of the ability to imitate supports the view of the presence of these practical ideas, and in the section of this paper on adaptive intelligence (p. 54 ff.) will be found additional evidence. The fact that only a few of the animals exhibited anything like the actions described, and each animal only a few, indicates that these 'practical ideas' play a subordinate and unimportant rôle in the ordinary life of the animal, but they do indicate that ideas may be present and have effect under certain circumstances.

Reasoning.

It was formerly held that man alone possesses reasoning power; other animals only instinct. Comparative psychology has modified the older view of instinct, viz., that it is a mysterious power, perfect at birth, unerring, unchangeable in its working, and radically different from intelligence. It has given the term a more precise definition, by limiting it in various ways, *e.g.*, from reflex action and habit on the one hand and the higher mental processes on the other. But comparative psychology has not materially changed the general view that reason is confined to man. And the generally accepted position is that the observed actions of animals may be explained as the results of simple associations. Some experimentalists have found material facts that indicate to them reasoning ability on the part of some animals, but this conclusion is not accepted by others. Here again, as in considering the subject of ideation, it is necessary to define what we mean by the term reason. This word has been used in a variety of ways and the following definitions include the most important of the meanings that have been given to the term. A, implied reasoning (Harris), *e.g.*, my recognition of yonder horse; B, inference from particular to particular (James), *e.g.*, the bird which finds bread upon the window one morning comes back the next morning; C, adaptive intelligence, the ability to adapt to our purposes conditions more or less difficult and more or less unfamiliar;

D, analogical reasoning, which involves construction or creation, *e.g.*, to reach an upper window I utilize a ladder which I find; E, rational thinking (James); F, formal or syllogistic reasoning.

That the higher mammals possess the ability to reason in the first two senses, probably no comparative psychologist will deny, although the explanation of the process may differ. Implied reason is probably a function of all animals and the ability to infer from particular to particular is well shown in all the experiments in habit production. It is the higher level of constructive analogical reasoning concerning which there is dispute. Has the animal power to create or to construct? Hobhouse in some very interesting experiments, claims to have found satisfactory evidence of the presence in animals of this class of reasoning. He calls the mental states that lead to this form of reasoning articulate ideas, and he has satisfied himself that these are present in some monkeys and apes.

With the purpose of confirming the work of Hobhouse I made experiments on three monkeys which had been extensively used in the previously described work on discrimination, etc., and with which I found it most easy to experiment.

The first experiment was one similar to the box and the chair experiments of Hobhouse. Food was suspended by a string from a long pole reaching across the room, too high for the monkeys to reach or to grasp by standing or by jumping. A light box was placed near the point of suspension, but sufficiently far away that the animal could not reach the food by standing upon the box. Only when the box was moved and the animal climbed upon it could the food be secured by the animal. Each of the three monkeys were given three trials, in each of which five minutes were allowed for the performance of the act. At the time the experiments were made the animals were hungry and apparently each watched me attentively when I fastened the piece of banana to the end of the string and suspended it. When all had been arranged the animal was permitted to approach the food and to secure it if possible. The actions of the animals were similar. Sometimes the animal tried to reach the food by jumping, sometimes after the unsuccessful attempts the animal would have a puzzled look and finally gave up attempting to secure the food. None of the animals seemed to notice the box and none made the attempt to use it as a means of reaching the food. In their actions there was nothing that I could interpret as a sign of deliberation or reflection, and in this experiment I was unable to verify the results of Hobhouse. It is possible that the box appeared too heavy for them to move, but I did not notice any indication that it had been observed, or rather observed in connection with the food. That the box was not too heavy was shown by the fact that a similar and equally heavy box was being constantly moved by monkeys in a large adjoining cage. Failure to secure evidence of reasoning in this experiment may have been due to the fact that I did not continue the experiment so long as did Hobhouse.

A second experiment was then tried. This experiment has been described in connection with the observations on imitation, and the apparatus is illustrated on page 42, fig. 2. The animal had to move a long pole, pivoted at one end, under the suspended food in order to secure the food. In all the experiments of this kind (three for each of the three animals) there was no evidence of ability to grasp the situation and to solve the problem. In one trial an animal climbed upon a cage which was near and jumped from it to the food. In general, however, each animal made unsuccessful attempts, most of them directed toward the food, and in each test finally appeared to give up trying. Two months later one of the animals (monkey 3) was again given three trials previous to the imitation tests and failed.

Adaptive intelligence.

Although no direct evidence of analogical reasoning was obtained in the tests just described, other experiments on reasoning gave interesting results. Those which are recorded in this section deal with adaptive intelligence, the reason as defined in the third class.

A piece of twine was permitted to hang in front of and 12 inches away from the cage, beyond the reach of the longest armed animal which was tested. At the end of the twine a piece of banana was arranged; a thin piece of wood was pushed through the banana and turned so that one end could be grasped by an animal in the cage. By grasping and pulling the stick inwards the food was secured. All the animals were tested in this experiment. The results with all except one of the animals were similar, and a description of the actions of one will suffice to indicate the whole. As soon as the banana and stick were arranged monkey 6 put her arm through the wire of the cage, seized the end of the stick, drew it toward her and secured the banana. This experiment was repeated a number of times and in all there was a similar immediate characteristic response. There appeared to be a decided adaptation of means to end. No efforts were wasted upon random movements. It did not appear that any preliminary attempt was made to grasp or even to reach for the food, but there was an immediate movement toward the stick. The results for all animals are given in table VIII, in which is shown the approximate time for the performance of the act by each animal in each trial. The absence of hesitation, the direction of the movement away from the food and towards the stick, and the promptness with which the food was secured speak for the presence of adaptive intelligence in ten of the monkeys. It would seem that this is almost always found in these animals.

In this test the results with monkey 5 were decidedly different from those with the other animals, in that he failed in the trials given him. Monkeys 1 and 4 had considerable difficulty in getting the food in the first and second tests, but there was no gradual acquisition of the method of securing food with the other eight animals. The times for solving the problem in the second and third tests were approximately the same as those in the first tests for monkeys 2, 6 and 10, and there was not much difference in the time between the first and the later tests for monkeys 3, 7, 8, 9 and 11. Much, if not all, the difference in time can be accounted for by the better adjustment to the wire netting of the cage, the pushing of the hand through in the proper place, etc.

TABLE VIII.

Adaptive intelligence, suspended food and stick. Three or six trials each animal. Time in seconds; f = failed.

TRIALS AND ANIMALS	1	2	3	4	5	6
1	60	10	3	—	—	—
2	4	3	3	—	—	—
3	20	6	9	—	—	—
4	105	40	11	—	—	—
5	f.	f.	f.	—	—	—
6	2	1	1	—	—	—
7	6	4	3	1	1	1
8	6	4	4	3	3	3
9	5	3	3	3	4	2
10	2	1	1	1	1	1
11	5	5	3	3	3	3

A second test of the presence of adaptive intelligence was made as follows: A light wooden lever *B*, 18.5 inches long was attached by leather hinges at one end to a board *A* which rested upon the floor The hinge of the lever was 4 inches from the end of the horizontal board, well within reach of the animals. The lever was inclined at an angle of approximately 45 degrees from the horizontal, and could be moved forwards in a vertical plane. Fig. 3 illustrates the apparatus in relation to the front of a cage. The apparatus was placed outside a cage, the lower end of the lever being within, the upper being beyond the reach of the animal. A piece of banana or other food was placed at the farther end of the lever, and the problem for the animal to solve was how to secure the food which was beyond direct reach.

Eleven monkeys were tested with this apparatus, and all with the exception of monkey 4 succeeded in the first test. In the first test the animals usually took a longer time to get the food, but as in the previous experiment this delay was largely one of making the adjustment of hand to the proper opening in the wire netting and not to attacking the apparatus in the proper manner. In the case of monkey 4, he appeared frightened at the time of the first trial and I showed him that food was to be secured by moving the lever against the cage and permitting him to secure the food. In the succeeding trials he immediately attacked the lever and obtained the food in the same manner as the other animals. Monkey 9 was in the same cage as monkey 8 when the latter wasbeing tested and may have taken the opportunity to observe monkey 8. That she performed the trick may have been due to imitation, but I have credited her with having performed it in the same way as the other animals. It is reasonable to suppose that monkey 4 would have been able to

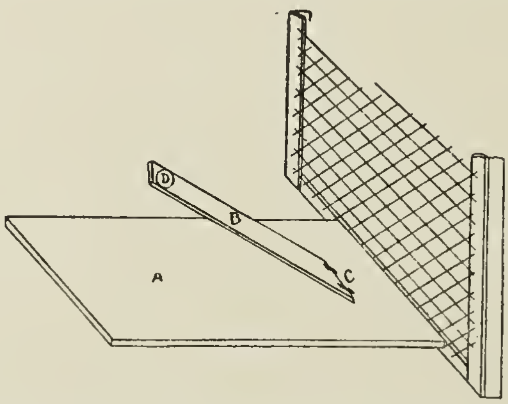

Fig. 3. Lever apparatus for test of adaptive intelligence C, leather hinges; B., lever; D, food.

manipulate the apparatus without being shown if sufficient time had been allowed. However, in whichever way it be conceived that the animal performed the trick we have a mental something very much like imitation or like adaptive intelligence. In many of the experiments the animals did

TABLE IX.

Adaptive intelligence, lever test. Time in seconds; f = failed.

TRIALS AND ANIMALS	1	2	3
1	9	4	3
2	6	4	3
3	$2\frac{1}{4}$	2	2
4	f	6	3

not use both hands for the moving of the lever, but one hand for the lever and the other for securing the food when the upper part of the lever was within reach. Table IX gives the time records of four monkeys in this experiment, those for the other animals were approximately the same as those noted in the table, although no accurate measurement by watch was taken.

A third experiment was then made. A stick 22 inches long, with 1 inch extending into the cage and the other outwards from the cage at a right angle, was arranged so that food could be secured by drawing the stick lengthwise into the cage. Ten monkeys were tested in this manner and all immediately appeared to grasp the situation for they pulled the stick and secured the food within three seconds. In the cases when the food was dislodged the animals immediately gave up the stick and turned to the food, an indication that the stick had been recognized as a means of obtaining the food.

During the progess of some experiments I noticed that monkey 7 attempted at one time to pull toward his cage a small tin bucket in which water was usually carried to the animals which had been unintentionally left near the cage. From this hint the following test was made with nine of the animals. One end of a piece of twine, 1 yard long, was attached to the bucket and the other end was left lying within the cage. The bucket was placed at the length of the twine away from the cage, and in it was placed a piece of food. In all cases the animals seized the twine immediately, drew the bucket toward the cage and seized the piece of banana. Table X gives the times for monkeys 7 and 8. These times are similar to those of the other animals.

TABLE X.

Adaptive intelligence, pulling bucket inwards by means of twine. Time in seconds.

TRIALS AND ANIMALS	1	2	3	4	5	6	7	8	9	10
7	6	4	3	3	4	3	3	4	3	3
8	0	3	3	2	2	3	2	2	2	3

A variation of the problem was then made. The end of the piece of twine which had been left lying loosely within the cage was tied to the wire netting 6 inches from the end, leaving that amount of twine within the cage. In this test the animal had to reach beyond the wire netting, seize the twine and pull the bucket. Seven animals were tested in this way by an assistant, and the notes are not so full as I could wish, but in brief the results are as follows:

Monkey 1,	first trial,	3 sec.; average	for 10 trials, 3 sec.
Monkey 2,	first trial,	20 sec.; average	for other 9 trials, 3.5 sec.
Monkey 3,	first trial,	20 sec.; average	for other 9 trials, 3.5 sec.
Monkey 4,	first trial,	5 sec.; average	for 10 trials, 3 sec.
Monkey 6,	first trial,	4 sec.; average	for 10 trials, 2 sec.
Monkey 7,	first trial	3 sec.; average	for 10 trials, 2 sec.
Monkey 8,	first trial,	120 sec.; average	for last 5 trials, 5.5 sec.

In the case of the animals whose actions were noted, each pulled at the short free end of the twine at first, but soon reached beyond the knot and pulled the bucket inwards. This was done by some of the animals, notably monkey 8, for four or five trials. The appreciation of the knot condition and the ability to deal with it is plainly shown by the records given above.

The results of all the experiments described in this section appear to speak for the possession by monkeys of adaptive intelligence, of a form of reasoning. The times for the performance of the various acts are fairly conclusive, but in addition the appearances of the animals, their actions, etc., especially during the first trial in each experiment indicated an understanding of the problem. There was no fumbling with the apparatus, no appearance of learning by trial and error, but there was instant action following apparently instant understanding of the situation. The reactions of the monkeys in the situations provided for them gave diversified and relatively abundant indications of the presence of practical ideas such as have been referred to in the section of this paper dealing with ideation (p. 40 ff.).

Memory.

No one questions the fact of the possession by animals of a sort of memory for acts once or often repeated. Memory in the restricted psychological sense, however, has been denied to be present in animals but it is obviously difficult, if not impossible, to determine this. The nature of retention and recall are matters that are disputed, but that animals have an organic memory, a physiological sort of memory, is admitted. This memory acts well for practical purposes and may therefore be called practical memory. Whether or not this be merely organic, or whether or not animal memory contains a representative factor must be left for future investigation.

The monkeys observed in this study showed good ability of recognition and retention, of the practical memory. After the completion of the tests on visual and auditory discrimination no further experiments were performed with the apparatus, until many of the later experiments on imitation, ideation, and reasoning were finished. Memory tests were then made of the animals previously used in the discrimination tests with the following results.

Visual discrimination of pink, yellow and green rice: monkey 1 took the green ten times, took the pink the first trial but only smelled it, and on the fourth trial took the yellow after the green but dropped it almost immediately; monkeys 2 and 8 took only green and paid no attention to the pink and yellow rice; monkeys 3 and 7 took green each trial, and in the first trial took pink after the green and smelled but did not eat it. Auditory discrimination of noises after thirteen days. Monkeys 4 and 6 made no mistakes in ten trials each.

Lever test of adaptive intelligence. Monkey 6, after 130 days, showed perfect retention in four trials; monkeys 7 and 8, after 123 days, were also perfect in four trials.

General Summary and Conclusions.

Monkeys learn to discriminate brightnesses, but take a long time for this when the stimuli have not a direct relation to the incentive for work; only a few trials are needed when the visual qualities are a part of the objects to which they naturally pay attention.

Colors are discriminated with accuracy and rapidity when the colors are parts of the food (red, pink, yellow and green).

Three animals gave clear indications of the discrimination of different degrees of noise, and also learned to discriminate musical tones.

The habits are formed rapidly if there be the double incentive of pleasant food as an inducement to a correct response, and of an unpleasant stimulus to check a wrong response.

From the experiments recorded in this paper it appears that monkeys learn to inhibit recently formed habits of action with facility.

As far as the evidence goes, in regard to both the formation and the inhibition of habits monkeys are superior to raccoons and far superior to dogs, cats, elephants, otters and other mammals which have been experimented with.

Monkeys have a practical memory; they appear to show a good degree of retention; the representative function in memory is an unknown quantity.

Of the higher powers of mind the monkey has only rudiments. He has a something which corresponds in function to ideas of a low order and which serves practical purposes. This something we call, with Hobhouse, practical ideas.

Two of the monkeys learned by imitation, but six others gave no indication of imitation ability. It may be said that while monkeys may learn by imitation to a limited extent, imitation as involving ideation is a small factor in their ordinary learning process.

All the tested animals appeared to reach a generalized mode of action in dealing with problems but there seemed to be no evidence of true general notions. They have an adaptive intelligence, a lower form of reason, or a mental state inferior to true reason.

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